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AN
EXPERIMENTAL ENQUIRY
INTO THE NATURE OF
GRAVELLY AND CALCULUS CONCRETIONS,
IN THE HUMAN SUBJECT;
AND THE EFFECTS OF
ALKALINE AND ACID SUBSTANCES ON THEM,
IN AND OUT OF THE BODY.
BY THOMAS EGAN, M. D. M. R. I. A.

READ, MARCH 6th, 1805:

THE constant occurrence of these afflicting complaints, in Simpson's Gouty Hospital, to which I have been physician for several years, first turned my serious attention to the most probable means of alleviating or removing them. But, to obtain this desirable end, an examination into the nature of the predisposing and proximate causes; of the chemical and other properties of gravelly matter itself; and that species of calculus, most generally resulting from its aggregation; as well as of the remedies, and their mode of operation; became indispensably necessary. I must also acknowledge, that I was not a little excited to this enquiry, by the consideration, that, whilst the medicines, now most confided in, by modern practitioners, are

supposed to exert no energy on those substances, out of the body; yet their beneficial effects, taken internally, stand uncontroverted, by the experience of almost every physician.

Induced by these motives, I had, as far back as the year 1799, instituted a series of experiments, in hopes of throwing some more light on this subject: and, perhaps, chemically explaining, upon what ground, alkaline substances in general alleviate, whilst acids as constantly aggravate, this afflicting disease.

But, knowing that Messrs. Fourcroy and Vauquelin had been, for many years, particularly engaged in the analysis of urine, and its morbid concretions; and expecting, from their superior abilities in researches of this kind, that the object, which I had in view, would be more satisfactorily fulfilled; I did not wish to intrude any observations of my own on the public.

After, however, most anxiously attending to the result of their scientific labours on this subject, as they have been, since that period, successively detailed by M. Fourcroy, in the *Annales de Chimie*, *Memoirs of the National Institute*, and latterly, in his great and elaborate work, the *Connoissances Chimiques*; and finding little, if, indeed, any thing, illustrative of the subject, to which I would wish to point the attention of the Faculty, as well as the public in general; I again latterly repeated, with much care, my experiments of 1799, and added some more,
which

which may probably prove interesting, in a practical point of view.

These, with some observations, and deductions from them, I now, with diffidence, offer to the candor and consideration of the Academy.

I must here premise, that the limits of an academic dissertation necessarily confine me, chiefly, to the consideration of gravelly matter itself, and that species of calculus, which most generally results from its aggregation.

Though determined to intrude as little as possible on their time, by an useless quotation from ancient authors, who could have no clear ideas of the subject: yet the better illustration of my object, as well as a sense of justice, oblige me to go as far back as Van Helmont, whose great, though eccentric genius first observed, that the subject matter of calculus existed in the urine itself. But the flighty extravagance of his ideas, of which he has given us a specimen on this subject, in his *Treatise de Lithiasi*, (a wonderful production for the time,) caused little attention to be paid to his opinion; and it was reserved for the capacious and learned genius of Boerhaave, first to ascertain, beyond future doubt, the presence of gravelly matter, as a natural constituent part of urine, kept in chemical solution in it, and eliminated by it, out of the system. Of this important fact, no material use was made, until the all-prying genius of the immortal Linnæus induced him to request his friend Scheele, to turn, for a moment, his great chemical abilities, to the investigation

of this subject; with what success is but too well known. And from this again had arisen the further prosecution of this enquiry, by the celebrated Bergman.

The result of the analysis of the latter was highly honourable to the former chemist; as they perfectly agreed in almost every particular, with the exception of some small quantity of insoluble matter, and the presence of lime, observed by Bergman: a difference now very easily accounted for; the former having examined calculi of the pure lithic acid, or, as it is now termed, uric kind, (by far the most common species,) and entirely soluble in pure alkaline lixivium, and nitric acid. The latter, those of the mixed kind, consisting, also, chiefly, of lithic acid, but with interposed laminæ; or, probably, a nucleus of either calcareous phosphat, or oxalt of lime, which frequently occurs, in a very large proportion of these concretions. We may also observe, that Bergman had not, at this period, an adequate idea of the large proportion and insolubility of animal matter, contained in them.

From their joint analysis, it was, for the first time, proved, that the subject matter of gravel, and of a very large proportion of calculi, was present, in a state of real chemical solution, in all healthy urine; that it was possessed of the following distinguishing chemical properties.

Insipidity, inodorous, crystallizable, nearly insoluble in cold water, and only soluble in some thousandth parts of its weight of boiling water: separable again from
this,

this, upon cooling, in a beautiful and peculiar crystalline form, of easy solubility, in pure alkaline lixivia, which it renders sweetish, and neutralizes: precipitable from these again, by the weakest acids, and still possessing its original crystalline form and properties. That, from these circumstances, with that of turning the vegetable blues *red*, it was of an acid nature, soluble in nitrous acid with effervescence: this solution tinging the skin, and other animal matters, red, and, upon evaporation to dryness, assuming a red rose colour: this last property being peculiarly characteristic of this substance; subliming in part by distillation, without any alteration in its properties, and affording carbonate of ammonia, and other usual animal products, partly from the admixture of animal matter, and propably some adhering urea. To these distinguishing chemical properties of the Swedish chemist, Fourcroy has since added the following. When triturated with a lixivium of either of the fixed alkalies, it forms a matter of a saponaceous consistence, very soluble with excess of alkali, but little so without it. The saturated urats of pot-ash and soda are little sapid, soluble, or crystallizable. By precipitating their dilute solution, by muriatic acid, we obtain the lithic acid, in brilliant, needle-like crystals, very voluminous, a little coloured, tending to the yellow, or *fauve*, (as he calls it). Ammonia exerts little, if any, solvent power upon it: lime-water takes up a little. The alkaline carbonates have no action upon it: and this last circumstance, I would beg leave to observe, has continued to
be

be the opinion, to this day; but how far founded, will appear in the sequel. To this matter Scheele gave the name of lithic acid; by which it continued to be known, until our countryman, Dr. Pearson, has latterly proposed that of uric; a change greedily adopted by the French chemists, as being more particularly indicative of its origin. And, though I would presume to think, that the former known appellation might be retained, without danger of leading into error, as to its nature; yet, in compliance with the philosophers of both nations, I shall, in future, term it uric acid, and the concretions of that nature, calculi of the *uric acid* kind. We must naturally suppose, that the publication of Scheele's Essay excited the experimental enquiries of both chemists and physicians. His experiments were, accordingly, repeated, by several of our countrymen in particular; but with various, and, in many instances, different results.

It was already cursorily observed, that Bergman's analysis differed from Scheele's, in some circumstances, which he, even at that period, was disposed to attribute to a difference in the nature of the calculi, which they respectively examined; and this conjecture has been fully established, by every subsequent inquiry, since that time. We accordingly find a paper of Dr. Dawson's, in the London Medical Transactions for the year 1769, shewing these concretions to be of very different and opposite kinds, and, of course, soluble in very different and opposite kinds of menstrua: as also a letter from Dr. Saunders to Dr. Percival,

Percival, of Manchester, published in the third volume of Percival's Philosophical and Experimental Essays, in 1776, detailing several experiments; from which he fairly concludes, that the Doctor's enthusiastic hope, of dissolving all calculi, in a solution of carbonic acid, must prove groundless, from the very different nature of their component parts, as ascertained by his own experiments. This was placed beyond further doubt, by our own learned and ingenious professor, Mr. William Higgins; who, in an analysis of a calculus, of which he gives an account, in his comparative View of the Phlogistic and Antiphlogistic Theories, (a work of singular merit for that period, to which we will afterwards refer,) and published so far back as 1789, enumerates the many various substances contained in one specimen only. The researches of Austin, Lane, and Brugnatelli, led to similar results. But to the learned and accurate Dr. Wollaston we stand indebted, for the first clear and distinct discrimination of the component parts of these substances. In a paper, read to the Royal Society in the year 1797, which would not discredit either a Bergman or a Klaproth, he has most accurately demonstrated, both analytically and synthetically, the component parts of three distinct species of calculi; namely, the fusible, as he terms it, or the ammonico-magnesian phosphat of Fourcroy; the mulberry, or oxalate of lime kind; and bone earth calculus, or phosphat of lime; which, with the uric, well known to us since the time of Scheele, left us then acquainted with the four species of calculi, of most frequent occurrence.

Under

Under these circumstances, I cannot help expressing my surprise, at finding M. Fourcroy still assuming the merit of the discovery, of all the different component parts of calculi; the uric acid, and phosphat of lime, excepted. This circumstance must appear the more unaccountable, when we consider, that the communication of Doctor Wollaston's experiments, was through the medium of the Transactions of the Royal Society for 1797. Finally, M. Fourcroy, to whom Europe stands not a little indebted, for the present general diffusion of chemical knowledge; and to whom the medical profession owe the greatest obligations, for his unremitted application to animal chemistry, has, in conjunction with Vauquelin, given us the result of his researches upon five hundred calculi: from which it appears that they contain the seven following ingredients.

1. Uric acid.
2. Urat of ammonia.
3. Phosphat of lime.
4. Ammonico magnesian phosphat.
5. Oxalt of lime.
6. Silica.
7. Animal matter.

From the prevalence of any of these ingredients, or their relative proportions, he divides them into four genera; and these again into twelve species: for an account of which I must refer to the tenth volume of the *Connoissances Chimiques*,

Chimiques, and the Memoires of the National Institute; not proposing to go into their chemical properties, farther than may be necessary to my present inquiry; namely, of how far acids may be conducive to the formation, or alkaliescent substances to the prevention, or even solution, of a large proportion of gravelly and calculus concretions. We have already remarked, that, to the sagacity of Boerhaave, we are indebted, for the knowledge of gravelly matter being a constituent part of urine, kept in chemical solution in it; and, happily for mankind, only separable from it, after being some considerable time out of the body. After minutely detailing the ingenious means made use of by Boerhaave, to ascertain this important point, to which I beg leave to refer, his commentator, Van Swieten, goes on to observe.

“ Hoc calculi rudimenta adsunt etiam in urina hominum sanissimorum; quæ, si una cum urina secernuntur, antequam ab urina secesserint, et concreescere inceperint, nullo modo sanitatem lædent. Cum autem observatum fuerit, illam separationem rudimentorum calculi citius fieri in quibusdam hominibus, tardius in aliis, patet, illos magis calculo obnoxios vivere, in quibus citius hæc separatio arenularum obtinet. An quandoque illa separatio contingit jam in renibus, et in vesica, antequam urina expellatur de corpore? Certe videtur. Vidi sæpius, una cum urina excretum sabulum nephriticum expulsum fuisse, statimque, calente adhuc et fumante urina, in fundo matulæ subsedisse. Contigit aliquoties, inventam

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“ fuisse

“ fuisse, in linteis sanorum infantum urina madidis, co-
 “ piam sabuli nephritici, satis duri, quod videtur una cum
 “ urina excretum fuisse. Cum enim magna cura habere-
 “ tur, ne hi infantes, (illustri genere nati,) diutius urinâ,
 “ vel aliis sordibus, conspurcati manerent, et urina statim
 “ per lintea penetret, vix videtur possibile fuisse, ut in
 “ urina jam emissa hoc sabulum productum fuerit, intra
 “ unam alteramve horm.”

And, again, he adds, “ Hoc sabulum, in urina etiam
 “ sanissima concrescens, vocari posset calculus nativus;
 “ a quo nemo liber est; at qui tunc tantum metuendus
 “ videtur, si cito in urina concreseat. Felices illi, in qui-
 “ bus tardissime hoc fit. Propriam sæpius examinavi uri-
 “ nam, lætusque vidi, rudimenta illa prima caculi sepa-
 “ rari quam tardissime, requiri quandoque horas viginti
 “ quatuor et ultra, antequam in sabulum majoris molis
 “ concreocere potuerint. Sed et, licet decimum tertium
 “ ætatis lustrum emensus jam fuerim, ab omni lithiasi
 “ immunis vixi.”

The mode and appearances, attending the separation
 and crystallization of this substance, from healthy urine,
 is one of the most beautiful, that, probably, chemistry
 affords. But, as the circumstances are so minutely and
 correctly detailed by Boerhaave, and his commentator, Van
 Swieten, in his treatise *De Calculo*, Vol. V. page 201 and
 202, and correspond so much with my own experiments,
 so often repeated; I must refer to him. On this passage,
 however, I must observe, that the space of twenty-four
 hours,

hours, mentioned by him, as the period of spontaneous separation, is by far, in the healthy state, too short, and that it extends to two, three, and, sometimes, more days, according to the existing temperature, and other circumstances. Nothing, therefore, I will presume to say, is more erroneous, than the assertion, repeated in almost every chemical book, that the uric acid separates from urine, upon cooling. When this occurs, which frequently happens, particularly with children, the urine is certainly surcharged with this very insoluble substance.

An increased temperature hastens the incipient decomposition of urine; and its first ammoniacal degeneration is always attended by the deposition of its uric acid, in its crystalline form.

This did not escape the observation of Hales, who tells us, that urine, tending to putrefaction, affords most of this acid substance: and, indeed, were it to be deposited upon cooling, or within the space of twenty-four hours, or even more, as is so generally asserted, it should, every day, present itself to physicians, who so constantly attend to the state of urine in glasses; but this is by no means the case: and we find Fourcroy, in his last publication, mentioning from twenty-four to forty-eight hours, which certainly only applies to summer heat, or the circumstance already mentioned.

Our next great obligation is, undoubtedly, to Scheele; who has made us acquainted with its nature, and the very distinct chemical properties already enumerated.

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While

While, in the state of gravel, it is ever the same, whether passed immediately with the urine, or spontaneously deposited, or precipitated from it: a circumstance that, for a long time, continued to give me much surprize, considering the variety of calculi; but of the truth of which I was convinced, by the examination of many hundred specimens, for many years back.

I was, therefore, pleased to find, that Fourcroy, for the first time, in his *Connoissances Chimiques*, asserts, “*les sables des reins sont presque toujours de l'acide urique.*” And, in another place, he says, speaking of the uric acid, “*cèt lui qui forme les sables, qui se crystallize et s'at-tache aux parrois de vaissaux.*”

No wonder, then, that calculi of this kind should be of most frequent occurrence; and that, of five hundred, analyzed by Fourcroy, one fourth should entirely consist of it, besides its occasional admixture with the remainder: and, of three hundred, examined by Pearson, the greater number were found to be of this nature.

Having premised these necessary observations, we have now to consider, to what circumstances we may attribute its separation, in a crystallized or aggregate state, from its natural solvent: the only condition, in which it can be productive of inconvenience, or diseases of this kind. And first; I would observe, that, being a natural secretion, of which the urine is only the vehicle, destined to carry it out of the system, it must be subject to the same derangements, with the other secretions of the human
body;

body; and may, of course, sometimes exceed in quantity, and, at other times, be more deficient; which last circumstance seems to take place, during the continuance of acute diseases.

That a morbidly increased secretion does frequently occur, and that, too, independent of external causes, we have the most satisfactory proof of, in the hereditary dispositions of many families to this complaint. And, indeed, when we consider the same to take place, relative to the functions, and secretions of the liver, we must not be surprised at similar deviations in those of the kidneys. Here, truly, they are of more mischievous tendency, as, from the very sparing solubility of the uric acid, (even in its own natural menstruum,) the smallest excess in quantity must subject it to precipitation.

Having premised these necessary considerations, I shall proceed to enquire into those circumstances, which the experience and observation of all times have pointed out to us, as the most frequent occasional causes of these maladies; and how far these opinions may be confirmed by experiments, instituted for that purpose.

And first. It is a matter of notoriety, that the period of life, from infancy to about fifteen inclusive, is most subject to disorders of this kind.

Of this practical observation, we have an interesting confirmation, inserted in the second volume of the *Memoires of the French National Institute, Mathematical and Physical Sciences*, year 7. Under the former happy regime,

gime, there was instituted, about forty years ago, at Luneville, in Lorraine, an hospital for the exclusive relief of calculus and gravelly patients. In that interval, sixteen hundred and twenty-nine, of both sexes, were admitted, and operated upon. Of these, fifteen hundred and sixty-four were males, and only sixty-five females.

Citizen Saucerotte, an associate of the institute, to whom we are indebted for these interesting details, annexes tables indicative of the number of these patients, that occurred at the different periods of life, from the age of one, up to seventy-eight. To these, as too extensive to be inserted here, I would beg leave to refer; and shall satisfy myself with some extracts only, expressive of the general result.

AGE OF PATIENTS.	NUMBER OF PATIENTS.
MALE SEX.	
1 year to 2.....	1
2 years.....	14
3	79
4	131
5	145
6	147

From this age, which afforded the maximum of the number of patients, we find a gradual declension, as follows.

AGE

AGE OF PATIENTS.	NUMBER OF PATIENTS.
8 years.....	121
10	79
15	39
20	16
25	7
30	8
35	4
50	5
60	2
70	2
78	1

Of the sixty-five females.

AGE OF PATIENTS.	NUMBER OF PATIENTS.
1 year to 3.....	1
4	8
5	7
6	4
7	6
8	5
9	3
12	4
14	1

From which period, down to seventy-eight, there occurs
but one or two upon each year. From these then we learn,
how

how much more subject the male sex is, to those complaints, than the female; and the earlier periods of life, than the more advanced, For, among the males in the sixth year, we find 147; (the greatest number,) and among the females, only five at eight. From these periods, in both sexes, the numbers rapidly diminish.

These facts would lead us to conclude, that some physiological cause, peculiar to the functions of this early stage, may give rise to this difference; and I will not pretend to say, but this may possibly exist: but, when we consider, that, in every country, the infant poor are the greatest sufferers, we are induced to inquire further, and suspect the existence of some general cause, affecting and applicable to them all. That a similarity of diet, (in the children of this class of society, in particular,) must every where nearly take place, is evident: and that this is, but too often, of the kind most prone to the acescent tendency, such as pap, gruel, sour milk, &c.; all which it is not always in the power of the parents to renew, or administer, in a recent and sound state; an error not unfrequently occurring, from the negligence of nurses, even in the upper ranks, but irremediable in the lower: where this acescent tendency cannot be corrected, by the seasonable admixture of broth, or other light animal food; their unhappy situation confining them exclusively, like their cattle, to the sole use of vegetables, and the farinacea.

To pass on, from infancy, to the advanced periods of
life,

life, and begin with our own island; we find, that, considering the extent of our population, the disease is of relative rare occurrence. So much so, that the late Mr. Dease, (whose premature death we have still to deplore, as a national calamity,) with all his well deserved celebrity, as a lithotomist, never operated upon more than sixty. A small number indeed, when we consider, that the operation is seldom, if ever, attempted in the country. And why this should happen here, we shall be presently, perhaps, better able to judge.

The reverse of this occurs in the sister kingdom; and the Irish student feels astonished, at the frequency of the operation in all the London hospitals, though also performed in these of the more considerable country towns: and, upon enquiry, he finds, that a large proportion of these patients come up from the cider countries of Hereford, Devon, &c. And it must naturally occur to him, that the general use of fermented liquors of every kind, beer, cider, perry, and factitious wines, which prevail in England, renders the disease of more frequent occurrence there, than with us; the great mass of our people being deprived of these luxuries.

If we pass over to the Continent, we find our neighbouring provinces, Picardy, Normandy, and Brittany, in particular, still more subject to affections of this kind: so much so, that the late Mr. Dease could not give credit to the extraordinary number of patients operated on, in one year only, in the hospital of Rouen; though many must have, of course, repaired to Paris. The same, though, in

a lesser degree, takes place in Champagne; and it is almost unnecessary to observe, that the general beverage of the northern provinces consists of cider, or of poor wine, equally acescent in its nature, and prone to the acetous fermentation. The Champagne, though somewhat less so, is replete with carbonic acid gas, and disengaged tartarous acid; and though, in the more southern provinces, this malady cannot be considered as endemial, yet it is of frequent occurrence in the hospitals of Montpellier.

For, even in these favoured climes, where wine is of so little value, and withal so spirituous, the unfortunate peasant is obliged to content himself with an inferior quality, prepared by a second maceration of the *marc* of the grape, which he denominates *picquet*; a *patois* appellation, most happily applied to its highly acid quality.

In that once happy country, Switzerland, on the contrary, as Baron Haller assures us, the disease is by no means frequent, and chiefly confined to the children of the poorer sort; their mountainous and elevated situations affording them little or no vinous liquors: whereas their neighbours, the inhabitants of the Rhine and Moselle, as well as some tracts on the banks of the Danube, are peculiarly afflicted.

The truth of this observation we find confirmed, by the medical authors of all times. Silvius observes, “*Vina acida tenuia et Rhenana, magis nocere calculosis quam opima;*” and the same is particularly insisted on, in Dolæus’s “*Encyclopædia, Ephemerides naturæ curiosorum,*” and Rivinus’s “*Morbi endemici,*” &c. Now, the wines in
these

these countries are well known to be of an acid quality; and Hoffman asserts, and that too from experiments, that they abound in the tartarous acid; having found them to contain a double relative quantity of that in other wines; and to this we may add no small proportion of carbonic acid. Linnæus, in his dissertation “*De Genesi Calculi*,” inserted in the second volume of the “*Amenitates Academicæ*,” seems more particularly to point out acids, and acescent drinks, as the chief causes of calculus affections. He says, “*acida fermentescentia omnia calculum promovent; hinc vina acida genesi calculi magis favent, quam dulcia. Qui acida vina copiose ingurgitant, podagræ et calculo plus exponuntur, quam illi, qui terras calidiores inhabitant, et dulcia vina hauriunt. Nec mirum, cum vini Rhenani libræ quatuor destillatione dant spiritus acidi drachmas quinque; et vini Tocariensis præbet spiritus acidi tantum semidrachmam, teste Hoffmanno. Sannissimus quisque a potu acido sæpe stranguriam incurrit, eo quod ab acidis ingestis particulæ terrestres præcipitantur.*” And again; “*quin podagra igitur et calculus ab acido generentur, nullum est dubium, id etiam ab eorum communi cura, ad quam pergimus, luculentius patebit.*” Beverovic, *De Calculo*, 80, also observes, “*in nullo vino tantum tartari apud nos accrescit, quam Rhenano. De me ipso, quod etiam ex plurimis audisse memini, possum testari, nunquam Rhenanum assumisse paulo largius, quin copiose arenulas excernerem.*”

The reverse of all this is observed to take place, where

the use of wine is prohibited. Rivinus observes, that, in the city of Batavia, where the pursuit of commerce brings together a vast assemblage of the neighbouring Asiatic nations, whenever the disease occurs, it is almost always in the instance of some Hollander, who, in his passage to India, drank freely of bottled beer, and used sour crout. In Persia, the same author, in his excellent treatise, *De Morbis Endemicis*, observes, that whenever calculus affection occurs, either in Ispahan, or the provinces, it is, assuredly, in the instance of some Armenian, (fellows, to use his words,) who, in every latitude, drink more wine than water.

Again, in Grand Cairo, where the proximity of the Grecian islands, and ready conveyance by the Nile, render wine of easy acquisition, and drunkenness and public-houses as common, as in any city of Germany; we learn, from Prosper Alpinus, that the disease is of very frequent occurrence; for, besides a mixed population of Franks, Armenians, Arabs, &c. the Mamelukes, as well as many other Turks of the higher ranks, do not, in deference to the Mahometan law, refrain from wine. The Cyprian and Grecian wines, if not adulterated, or become acescent by dilution, and the warm temperature of that city, are, in themselves, among the least objectionable. But, when we consider, that Paris is chiefly supplied with Burgundy, and that, yet, in no part of the world does there occur more mischief, from the attempts to keep down and correct its acescency, we will easily form an opinion of the quality of the wine retailed in Cairo.

To

To this abstinence, then, from wine and fermented liquors; as also, perhaps, to the admixture of a large proportion of the warmest spices, in their vegetable food, tending to correct its acescent tendency; we may ascribe the rare occurrence of this disease, in the more southern climates.

Now, these more general remarks, we find peculiarly to coincide with the observations of the patients themselves, as well as that of the physician: for such as have laboured under these complaints, a sufficient length of time to become acquainted with the *juvantia* and *ledentia*, most scrupulously abstain from acids, and acescent drinks of all kinds, and, what they find most particularly pernicious, beer, or ales, turning over to the acetous fermentation, or *hard*, as they are generally termed. And, indeed, nothing is more common, than that an indulgence in cyder, claret, or acidulated punch, nay, a draft of hard beer or porter, should be followed by a fit of the gout and gravel.

The connection between these diseases forms an interesting and curious subject of physiological, as well as pathological inquiry: but, proposing to offer some observations on this subject on a future occasion, I shall, at present, decline entering upon it, and pass on to observe, that the bad effects of all acidulous drinks, are fully confirmed, by the experience of our many sufferers in Simpson's Hospital. Hewson, who lately died there, at the advanced age of 102, never tasted the beer of the house, during the summer months, and substituted milk to it;
being

being taught by experience, that its acid tendency, during that period, always induced his gravelly paroxisms. And Clapham, who suffered much from gout and gravel, and was, for many years, a ship-captain, informed me, his voyages to America were always succeeded by fits of both; which he attributed to a free indulgence in the use of cyder, a beverage to which he was then peculiarly attached: and that, at any time, he could excite a paroxism of one or the other, or both, by drinking acidulated punch, or claret. Khensk, our greatest martyr, (having all his articulations distorted by gouty concretions, and who once lived in easy circumstances,) assured me, that the severest and longest protracted fit of the gout and gravel, he ever experienced, was occasioned by a surfeit of a poor vapid claret. And I shall conclude this part of my subject, by observing, that the clergy of the Roman Catholic church are peculiarly liable to these complaints, and form no small proportion of the number operated upon in this city: which I would attribute to the use of a small and sour wine, during their residence in their seminaries abroad.

These facts being pretty well established and acknowledged, it is time to inquire, how far we may account for them; and whether experiments, instituted out of the body, may not throw some light on this subject. Dr. Saunders, in a letter to Dr. Percival, "Percival's Essays Medical and Experimental," Vol. III. on the subject of carbonic acid, as a solvent of calculous concretions, observes, "if
 " a more

“ a more powerful and active solvent, than any hitherto
 “ known, shall be discovered; it is highly probable, that
 “ such a discovery can only be made, by a rational and
 “ chemical inquiry, into the powers of different bodies of
 “ combining with the contents of the urine, and preserving
 “ them in a fluid state out of the body.” Now, on the other
 hand, we may presume, that, whatever substances cause a
 separation, or precipitation of uric acid, in an aggregate
 state, from healthy urine, will give rise to these disorders.
 For, we are not to forget, that the uric acid, which forms
 so large a proportion of caculous concretions, and the en-
 tire of the gravelly, is a natural secretion from the blood,
 performed by the functions of the kidneys, and excreted by
 the urine; and can only be prejudicial, by a previous
 morbid separation from it, within the body. With this
 necessary view of the subject before us, (for which we are,
 as already observed, indebted to Boerhaave,) I resolved
 to try, First, what might be the effects of acids of diffe-
 rent kinds, on healthy urine, as to their influence, in caus-
 ing this same previous precipitation; and, Secondly, that
 of alkaline substances, in preventing it. And, here, it must
 be observed, that, to draw any satisfactory conclusions,
 from experiments, made with these substances out of the
 body; we must suppose they reach the kidneys, and blend
 with the urine, still possessing their relative distinctive pro-
 perties: and, that this takes place, we have every reason
 to presume. Doctors Percival and Saunders, Mr. Bewley,
 and others, have ascertained the presence of carbonic acid,
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in an uncombined state, in the urine of those, who drank the mephitic water, for some days; an acid certainly foreign to its recent healthy state: for, after repeated trials, by heating it to nearly ebullition, in one of Priestley's air-bottles, I never could procure the separation, or transition of a single bubble of carbonic acid, into a jar of lime-water. And, if this weak acid reaches the kidneys, undecomposed or uncombined, we will have less difficulty in believing, the more powerful ones may do so. That the tartarous acid, in the combination of the acidulous tartarite of potash, exerts powerful effects on the functions of the kidneys, is well known; and, that the urine is, at the same time, rendered more acid, I have repeatedly ascertained, by the usual tests.

We may say the same of the other vegetable acids, which manifest also diuretic powers, and increase the natural acidity of the urine. Linnæus, in his second volume of the *Amenitates Academicæ*, *De Genesi Calculi*, already quoted, mentions his having made the following experiment to this purpose. He says, "*hisce diebus ipse experimetum institui cum urina; hæc communiter a solutione lacmus parum admodum rufescit; at si libram unam vel alteram vini Rhenani, vel alterius vini acidi hauserim, post horam unam vel plures, valde rubra et rutilans evadit urina, ab affusa solutione lacmus; certo indicio, acidum vini totum corpus permeasse, et urinam infecisse.*" Nor should we wonder, that these energetic substances should pass unaltered to the kidneys, when we find so many mild
vegetable

vegetable matters do so. I will not mention the communication of so volatile a principle as odour, but will more particularly dwell on that of colour. Rhubarb, turmeric, madder, and many other substances, so completely impart their colour to urine, that they would appear to be very little altered. Nay, the juice of the beta vulgaris, a mild esculent of the pentandrious class, so deeply reddens it, as to cause it to be mistaken for bloody urine, of which a late instance has occurred in my practice.

As to alkaline substances, it has been at all times known, that they communicate their properties to this excrementitious liquor. A perseverance in the use of the *aqua kali puri*, of the shops, for a few days, even in small doses, converts its acescent into the alkaline state; and we have every reason to suppose, that the same takes place with the carbonates, which are taken in so much larger quantities. This seems confirmed, by experiments made in London and Paris; and the alkalescent impregnation of the urine was ascertained by the formation and precipitation of the acidulous tartarite of potash, upon the addition of the tartarous acid. Yet, from a good deal of experience in these matters, I may aver, that, as to the carbonates, the dose must be considerable, (which was the case in London,) and continued for some time; having frequently given two scruples of desiccated soda, (containing, according to Mr. Kirwan, 23,94 grs.) in the twenty-four hours, for some days together, without any diminution of the usual acidity of this liquor.

For the information of such of my readers, as may not be of the medical profession, I must here observe, that physicians distinguish two kinds of urine: the one rendered immediately after meals, and much dilution, before the process of digestion, or state of sleep can take place; always more or less limpid; being comparatively less charged with the natural component parts of urine, (the urée, or extractive colouring matter, in particular,) and called *urina potus*, to distinguish it from the *urina sanguinis*, rendered, many hours after meals and sleep, the taking no more than a necessary quantity of liquids, and containing the usual proportion of saline, and other ingredients; more especially the urée, to which it owes its natural citrine colour.

This last, therefore, was that employed in the following experiments, if not otherwise specified: with the chemical history of which I must suppose gentlemen of the profession now tolerably well acquainted; being so fully and accurately detailed, in the tenth volume of the *Connoissances Chimiques*.

Having, in the preceding pages, insisted so much on the acids, and acescent drinks, as occasional causes of these complaints, the first object seemed to be, to ascertain whether the urine of those most subject to them, or actually labouring under them, was more relatively acid. We have already seen, from a register of these patients, kept for forty years in the hospital of Luneville, that the early period of life, from two to six years of age inclusive,

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is most liable to calculus affections. Now, the urine of healthy children is always found more acid than that of adults; generally, in the proportion of two to one. Whilst several drops of the latter are requisite to redden a given quantity of infusion of litmus, a single drop of the former turns it to a clear red. Paper, stained with an infusion of turmeric, and reddened by an alkali, was immediately restored to its colour, by a single immersion in the urine of children: an effect, which required some time in that of adults. And that this should be the case, we will not be so much surprised at, when we consider the nature of their diet; and that, in addition to the phosphoric and uric, their urine contains the benzoic acid in considerable quantity: the proportion of which is found afterwards progressively to diminish with their advancement in life.

The constant opportunity I have, of attending to those subjects, enables me to say, that the urine of gravelly patients, when fresh rendered, nay, after standing many hours, in a temperature of sixty degrees, is relatively more acid than the healthy; sometimes as much so as the gouty; and frequently continues so, even after depositing its gravelly matter. An exception to this, however, sometimes occurs in gouty habits; their urine depositing copiously this acid substance, and yet manifesting no increased, but sometimes rather decreased acescency: for, with them, a considerable diminution of the quantity of the usually excreted super-acidulated phosphoric salt often takes place, as shall be fully explained upon another occasion.

Having premised these observations, it is now time to consider what effects acid substances are productive of; when mixed, out of the body, with this very complicated liquor. And here, to prevent repetition, I will observe, that that generally used, was rendered fresh in the morning, in the quantity of from three to four ounces, (unless otherwise specified;) being that most easily retained at one time in the bladder. The quantity of acid extremely small, for obvious reasons, and seldom increasing its acescent properties (as ascertained by the usual tests) beyond what frequently occurs, in the urine of those who use acescent drinks, or are afflicted with gout or gravel. A standard quantity was always laid by for comparison; and the temperature from sixty to seventy-five degrees, being in autumn, 1799. And to begin with the vegetable acids.

EXPERIMENT I.

To four ounces of the urine of an adult, was added one drachm of common acetous acid, which (like every other acid) caused no immediate change in it; but, in a very short time, and before it cooled down to the temperature of the atmosphere, some extremely minute shining spiculæ, observable only by a lens, were seen floating in it: these gradually increased in number and size, began to reflect the light, and, from being perfectly transparent, soon became coloured, to settle upon the usual cloud, or *nubecula*, which now began to form, adhere to the sides
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of the glass, and partly fall to the bottom, in the shape of small bright red crystals. In the standard, after twelve hours, nothing more observable, than the usual *nubecula*; nor was there any sign of crystallization, or separation of uric acid, even after twenty-four.

EXPERIMENT II.

To the same quantity of adult urine, were added one drachm and half of acetous acid, which caused a more copious separation and crystallization of this substance, with the foregoing appearances. None observable in the standard after twenty-four hours.

EXPERIMENT. III.

To four ounces of urine of a healthy child, who never was observed to pass gravel, and of the usual degree of acidity, was added one drachm of acetous acid, which soon caused an evident and copious separation of crystallized uric acid. The crystals were, however, not quite so coloured; the urine of children not being so much impregnated with the urée, or colouring matter. No such appearance in the standard after twelve hours or more.

EXPERIMENT IV.

To four ounces of adult urine, rendered very soon after a tea breakfast, and nearly in a state of *urina potus*, was
added

added one drachm of acetous acid. After three hours, a crystallization of minute sandy particles took place. None in the standard, even after three days.

EXPERIMENT V.

Thirty drops only, of acetous acid, were added to four ounces of the urine of a gouty patient, æt. sixty, and who sometimes felt some slight gravelly tendency. A very copious precipitation of this matter quickly took place. Some observable in the standard, also, the next day.

EXPERIMENT VI.

To three ounces of healthy adult urine, were added a few drops only of citric acid. A distinct crystallization, but extremely minute, took place. No appearance of any in the standard, after many hours. The experiment was repeated with one drachm of filtered citric acid, which only hastened the separation, and increased the quantity of crystalline matter.

Finding, by these experiments, and numberless others, with a detail of which it would be unnecessary to take up the time of the Academy, that the acetous and citric acids, blended with the urine, separated its uric acid in a crystallized state; I thought it might be interesting, to investigate what the effect of the tartarous acid might be: being that, which, in an uncombined, and partly combined state of acidule, as in the acidulous tartarite of potash,
chiefly

chiefly prevails in the wines and beverage of those countries most subject to these complaints.

EXPERIMENT VII.

To four ounces of healthy adult urine, were added some drops only of pure tartarous acid. To the same quantity, one drachm of acetous acid; which brought them nearly to the same standard of acidity: a circumstance always attended to in the comparative trials with different acids. In that with the tartarous acid, the crystals were not only larger and darker coloured, but exceeded in quantity any thing before observed. In that with the acetous acid, a much smaller proportion of minute crystals took place.

EXPERIMENT VIII.

To four ounces of urine, were added two drachms of a filtered solution of acidulous tartarite of potash, of the temperature 55 degrees. The usual separation and crystallization took place, in large proportion: the crystals, however, much smaller, and less coloured, than those with the uncombined tartarous acid. The two last experiments, frequently repeated, presented the same results.

EXPERIMENT IX.

The result of the above experiments having led to some doubt, as to the good effects of the carbonic acid gas, so much,

much, at one time, recommended by Doctors Percival and Saunders, previous to its more modern alkaline combination, in our mephitic, as well as super-aerated soda waters.

Into the middle part of Nooth's apparatus, were introduced four pounds of fresh rendered healthy urine, and exposed to a stream of carbonic acid gas. After a few hours, a copious and beautiful precipitation of uric crystals took place, (notwithstanding the constant agitation, from the transmission of the gaseous bubbles,) larger than any I before observed, that from the tartarous acid excepted. In a standard quantity, no distinct crystallization, even after two days. A repetition of the same experiment afforded similar results.

EXPERIMENT X.

Finding the carbonic acid gas productive of similar effects, with the other acids hitherto examined; it was natural to inquire, how far its combination with the portion of alkaline matter, contained in our mephitic and soda waters, so highly surcharged with it, may prevent a separation of this uric acid.

Half an ounce, only, of the common soda water of the shops, prepared by Mr. Kinsley, was added to four ounces of healthy urine. A similar quantity was impregnated with carbonic acid gas. In the former, after forty-eight hours, or more, no more than the usual *nubecula*: nor could a single crystal be discovered, even by a magnifier. In the latter, an early, copious, and beautiful crystallization.

tion. On the result of this experiment, frequently repeated, with various proportions of the mephitic alkaline water, I shall afterwards have occasion to make some remarks.

Though the mineral acids, in an uncombined state, enter not into the matter of our diet, and are no longer considered as lithontriptics, since the notion of the earthy nature of these concretions has been abandoned; yet, as they are sometimes prescribed with other indications, I thought fit to extend my researches (though in a summary way) to them also.

EXPERIMENT XI.

To sixteen ounces of urine, were added eight drops of very dilute sulphuric acid. To a similar quantity, two scruples of citric acid, to bring them to nearly the same standard of acidity. After a very short interval, in that with citric acid, the usual appearance of transparent floating *moleculæ* reflecting light, and gradually becoming larger, were observed, and began to adhere to the glass: whilst in the other, after five hours, no such appearances took place. Yet, after forty-eight, here also a precipitation took place, of smaller crystals, and less in quantity; for, being collected on a filter, and carefully dried, they weighed only two grains; whilst the former amounted to three. And this is nearly the largest proportion I ever found the above quantity of healthy urine to contain.

EXPERIMENT XII.

As the nitrous acid is one of the most active solvents of this matter, out of the body, I was curious to ascertain, whether, in the very dilute state in which it must reach the kidneys and bladder, (where its action must have been facilitated, by the actual state of solution of this substance,) it would manifest its powers, in preventing its separation.

To three ounces of urine, rendered a few hours after breakfast, and, of course, scarcely acid, were added five drops of weak nitrous acid; which did not seem to add very materially to its acescent properties.

To a similar quantity were added four scruples of acetic acid. In less than an hour, the former deposited a distinct quantity of gravelly matter, in considerable proportion. This, perhaps, we should not be surprised at, when we consider how the action of this acid, in that fluid, may be determined by superior affinity. In the latter, the separation did not take place for a considerable time after. We see, then, that the nitrous acid speedily and powerfully precipitates this acid substance.

EXPERIMENT XIII.

To six ounces of urine, shewing a strong acescent quality, were added only three drops of strong marine acid.

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A cloudiness and transparent granular precipitation took place, followed by the formation of extremely minute gravelly concretions, which, even after two days standing, did not assume so red a tinge as that with vegetable acids. This may, probably, depend upon some action of this acid upon the urée, or colouring matter: but, as to the smallness of the crystals, that evidently depends upon the more speedy precipitation, throwing them down before they can assume their natural size, and leaving but a shade of difference between the crystalline and pulverulent deposits.

EXPERIMENT XIV.

From the above, then, we are satisfied, that the vegetable and mineral acids cause a premature separation and crystallization of the lithic contents of recent healthy urine: but it may be observed, that this only takes place, under circumstances not at all applicable to the living system; viz. a much inferior temperature; and, in some instances, a contact with the atmospheric air: two powerful promoting causes of crystallization in general, but more especially of the less soluble salts. To determine, therefore, this most essential point:

To six ounces of cold but recent urine, (in a well closed phial,) were added five drops of very dilute nitrous acid, which were placed on a sand bath: temperature varying from 80 to about 100 degrees at most. The same quantity, with similar precautions, but without addition, was laid

aside, in the laboratory, as a standard: temperature 56 degrees. After a very short interval indeed, and almost as soon as the urine acquired the temperature of between 80 and 90 degrees, small shining granular particles were observable with a magnifier, began gradually to settle upon a broken kind of *nubecula*, or rather *nubeculæ*, and to acquire colour and size, though carried up and down the liquor, which was in constant agitation. This experiment again twice latterly repeated, and always with the same result, (care being taken to keep the temperature, as nearly as possible, for a few hours, between 90 and 100 degrees,) afforded one of the most pleasing objects imaginable; viz. the formation of this crystalline matter, under all the disadvantages of elevated temperature, and constant agitation, from (I may almost say) their primordial *moleculæ*, to the accomplishment of their full size. And here, indeed, they are most beautiful, and not to be distinguished from these spontaneously deposited.

The whole experiment strikes us strongly with a semblance of what probably passes, under similar circumstances, in nature; and reminds us of the danger attendant upon acid impregnations, more particularly at bedtime, when the urine, by many hours retention and quiet, has ample time to deposit its uric acid contents in the bladder. From it, also, we learn, that the temperature of the human body, in place of retarding or preventing (as might be expected *a priori*) these pernicious effects, rather promotes them, and that to a considerable degree.

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But whilst we endeavoured to establish this point, from practical observation as well as experiment, we seem to have entirely forgot, that the urine itself is an acid liquor; and that, therefore, if acids were so prejudicial, it is not probable, that the provident wisdom of nature would commit the discharge of this necessary excretion to a fluid, which, by prematurely separating it within the body, would completely defeat the object of her humane attention. And would she not, in the infinity of her resources, dispose of it by some less objectionable emunctory?

I would, in the first place, observe, that though healthy urine manifests the properties of an acid liquor, it is in the very smallest possible degree; so much so, that though mentioned long since, by Moraung, Coldevillars, and other surgeons, yet it was not, either chemically or medically, acknowledged to be so, until the time of Scheele, who finally established this point, as well as the nature of the prevailing acid. And, secondly, that nothing can be more erroneous, than the opinion, which so long prevailed, that the phosphoric acid existed in it, in a naked or uncombined state. It is now well established, that it is only in that of a weak acidule, or acidulous phosphat of lime, very little short indeed of the point of saturation; and hence the weakness of its action, as an acid liquor: for were it not for litmus, and some of the more delicate of the vegetable blues, we would have been, even to this day, ignorant of this property; so very feeble indeed, that it will often not affect an infusion of red cabbage, whilst
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it turns with litmus, and, sometimes, but feebly, with this most delicate of all acid tests. A single drop of phosphoric acid was added to one ounce of distilled water. Of this weak acid impregnation, one drop was sufficient to turn the infusion of litmus of as clear a red as the mineral acids do; whilst seven of urine manifested but very weak effects of acidity, and required some time to shew any. If the urine, therefore, does not exceed its natural standard of acidity, we have nothing to apprehend. And here, indeed, we must again admire the wonderful wisdom of Providence. The occasion (may I be allowed to say so, and that, too, before so competent an assembly?) required some chemical discrimination. It was necessary to carefully provide for the expulsion of the recrementitious part of the osseous fabric (which is very considerable) out of the system: but as this salt is insoluble in an aqueous vehicle, such as the urine, nothing more would be necessary, to obviate this difficulty, than a certain degree of super-saturation, or state of acidule, which would more effectually provide for its solubility, and its elimination. But by going thus far, whilst it attended to one excretion only, it would have entirely forsaken its charge of another, committed also to this fluid; and, by this degree of super-saturation, precipitate, retain in the system the uric acid, and occasion as frequent an occurrence of gravelly and calculus complaints, amongst mankind in general, as now occurs among the gouty. It, therefore, prudently formed that degree only of acidulous phosphat of lime, which, though insoluble out
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of the body, was sufficiently soluble, when assisted by its temperature. Nay, even for wise purposes, it has given a degree of latitude to this temperature, which, though narrow and confined indeed, is sufficient for its purposes: but where it precisely terminates I am not at present prepared to say, though so easily determined.

Let us now, for a moment, consider how far any morbid deviation, from this healthy standard, (which sometimes happens,) may throw light on this subject. The most considerable, that I am acquainted with, occurs in the instance of gouty urine, rendered towards the decline of the paroxysm. A single drop of this, though in a turbid state, affects the vegetable blues, with an energy, equal, or, perhaps, superior, to that of the strongest acetous acid; and requires a very considerable increased proportion of lime-water to decompose it, for obvious reasons. This we find always depositing, sometimes from the bladder itself, but, generally, before it has entirely parted with its natural temperature, a very large proportion of a reddish brick-dust like sediment, (a welcome harbinger to gouty patients,) gradually declining, and keeping pace with the alleviation of symptoms, and the progressive return of the urine to its natural degree of acidity. This sediment, Scheele, Bergman, and Fourcroy, consider of the uric acid kind: and so it (but in part only) undoubtedly is; being in a smaller proportion than they were aware of. For, considering that the enormous quantity, rendered in a few days, was incompatible with the known minute proportion of this acid
matter

matter in urine, I was determined to make the following experiment: To a considerable quantity of it, desiccated and welledulcorated with distilled water, were added three ounces of a weak alkaline lixivium; which, after a few hours digestion, completely discoloured it, acquired a golden yellow colour, a sweetish taste, and, on the addition of a few drops of dilute marine acid, precipitated a copious sediment of whitish minute needle-shaped crystals, of a silky appearance.

To this precipitate, welledulcorated, was added, by degrees, about one ounce of weak nitrous acid, which acted on it, with effervescence, and nearly took up the whole. This solution, being set to evaporate, began to redden the fingers, and other animal matters; no doubt, therefore, could subsist, as to its nature. To the remainder, which seemed very little diminished, and only deprived of colour, were added two ounces of dilute marine acid; which, after some time in digestion, nearly dissolved the whole: and, finding this acid solution precipitate with lime-water, oxalt of ammonia, and fixed alkali, it must have been phosphat of lime. This forms, then, by far the largest proportion of the gouty sediment, which is coloured by the precipitated uric acid. Such, also, is the result of Crookshank's experiments; and so we should expect to find it, as I shall endeavour to point out, on a future occasion.

Let us now consider, how far these analytical results may be confirmed, in the synthetic way; having resolved,
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that experiment, as far as applicable, should form the basis of any opinions, offered in this essay. The phosphoric being the native acid, prevalent in urine, it was interesting to determine, whether, by the artificial super-addition of it, so as to bring this fluid to the standard of the gouty, we might not produce effects, somewhat analogous to what occur there.

Eighteen ounces of urine were divided into three equal parts. To the first were added, five drops of phosphoric acid; to the second, ten; and to the third, fifteen. In the first, the magnifier very soon discovered minute floating *moleculæ*, gradually assuming the crystalline form, &c., as so often before described. In the second, the same appearances, but more immediately and copiously produced. But in the third, so considerable, as to excite my astonishment. For here, besides the same extremely minute crystals, which adhered to the entire sides of the phial, the bottom appeared covered, with a mixture of crystalline, and red pulverulent matter: the latter in great proportion, and, probably, prevented from crystallization, by its hasty deposition. Here, then, that encreased proportion of calcareous phosphat and animal gelatinous matters, (which always takes place in gout, and could not be expected here,) would seem only wanting, to form a sort of synthetic approximation to the gouty sediment.

The unusual proportion of deposited uric acid, in this experiment, created some suspicion, that the phosphoric acid might, by a combination with some of the principles

of this very compound fluid, give rise to some artificial formation of it, on this occasion.

To the filtered liquor, therefore, of Number 3, were again superadded five drops, which, in twenty-four hours, caused a farther separation of a very few crystals only. It was filtered a third time, and eight drops more added; but without the smallest appearance of a single crystal, after four days. The additional acid, then, only more effectually and speedily determined the separation of the quantity, naturally contained in urine: its more divided pulverulent appearance adding considerably to its volume.

It now only remained, to demonstrate the identity of these various precipitates, with the naturally deposited matter of gravel. For, though it could not be well mistaken, for any other saline composition in urine; yet, as external characters are, even in the hands of a Romé de Lisle, or an Abbe Haiüy, fallacious, the following, and concluding one, on the subject of acids, was instituted.

EXPERIMENT XV.

To two drachms of this artificial gravelly matter, was gradually added one ounce of nitrous acid; which acted on it, with effervescence, and dissolved the whole, with the exception of some small, floating, flocculent, animal particles, so well described by Bergman.

The evaporated solution reddened the skin, and, after some time, deposited crystals of oxalic acid; as happens in
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all concentrated nitrous solutions of calculi, of the uric acid kind. To another small quantity, was added some pure alkaline lixivium; which very soon took it up, became coloured, sweetish, and deposited the usual silky crystalline sediment, upon the addition of acetous acid. No doubt, therefore, could remain, as to its identity, with that naturally deposited.

And here, though irrelevant to my present object, and merely with a view to excite the attention of the faculty, may I be permitted to ask, how it happens, that, in the very worst kinds of typhus fever, there is very little diminution of the secretion, or excretion of the acidulous phosphat of lime? as appears by the acidity of the urine, lime-water, and the *quantum* of precipitate, afforded by the oxalic acid: whilst a very considerable one of the uric acid takes place, and continues so, until nearly the termination of the disease, when it begins gradually again to manifest itself; first, by the usual tests only; but presently, upon the crisis taking place, in such quantity, as to become insoluble; and, therefore, quickly precipitates, (with some additional mixture of calcareous phosphat, and animal mucilaginous matter,) under the form of our critical sediment or deposit? Or, are we not here, again, to to admire the wise economy of the Author of nature, which, by keeping up the considerable and necessary bony excretion of the system, prevents the dangerous accumulation of it, which must ensue, from its retention, during the long protracted period of many fevers? I might

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here offer some conjectures, in explanation, but will reserve them for another place.

Having already trespassed so much upon the indulgence of the Academy, I shall here content myself with briefly stating, that, from the above experiments and observations, we may presume to say, acids of every kind are prejudicial, and give rise to the formation of gravelly and calculus affections, by causing a separation, and crystallization of the lithic acid contents of urine, within the body: not pretending, however, to deny the existence of other causes, inherent in the system itself, occasionally productive of similar effects, as has been already observed.

I shall now proceed to the second part of this inquiry, namely: how far, or in what manner, alkaline matters are conducive to the alleviation of these complaints.

PART

PART II.

THE bad effects of all acid and acescent substances, being generally felt and acknowledged, we cannot be surprised, that sufferers from these maladies should naturally expect an alleviation of their complaints, from substances of a very opposite nature; or that, perhaps, in the general anxiety of mankind to discover a solvent of these concretions, the active agency of alkaline matters could not be overlooked. We accordingly find, that, from the remotest antiquity, up to this day, they were, and still are, though under various modifications, chiefly resorted to. Our ancient physicians prescribed waters with mineral alkaline impregnation; such as Seltzer, Carlsbad, and others: and, in latter times, we find our own countrymen more particularly engaged in these pursuits. Lime-water, recommended by White, (to whose numerous and interesting experiments I must beg leave to refer): lime, and pure alkaline matter, forming the bases of the celebrated remedies of Madam Stephens, Hartley, and others. And, in our own days, the caustic lixivium, again forgot, to make room for the more modern and fashionable introduction of both our alkaline, sub and super-carbonates; the vegetable, as in Faulknor's mephitic alkaline water, or in the crystallized carbonate of potash; the mineral, in a desiccated

cated state, as recommended by my learned and indefatigable friend, Dr. Beddoes; or in that of the well known soda waters, first introduced in Geneva and Paris.

Now, in whatever of the above forms these saline matters are employed, their decided good effects are universally experienced and acknowledged. The *Aqua mephitica alkalina* I consider the most valuable gift, bestowed upon mankind, by our modern chemistry: and, to Beddoes' desiccated soda pills, my colleagues must join with me, in acknowledging our greatest obligations. But how account for these good effects? or what can their *modus operandi* be? *Hic labor, hoc opus*. Carbonates, we have always been given to understand, exerted no solvent power on gravelly or calculus matter: and this continues to be the opinion of philosophers, as well as medical chemists, to this day. We find Fourcroy, in his late elaborate work on this subject, still continuing to assert, (in mentioning the action of various matters upon uric acid,) "les carbonates alcalines n'ont aucune action sur lui." Nor does the difficulty diminish, with respect to the pure alkalies; for, in the stomach and *primæ viæ*, they must return again, to either a carbonated or saponaceous state. My ingenious friend, and master in chemistry, Mr. Wm. Higgins, (in the work already quoted,) emphatically exclaims, "why not at once give soap? why not turn our attention to the mild mineral alkali?" With regard to the common alkaline carbonates in use, it may be observed, that the saturation is not complete; and that the uncombined portion of alkaline matter may still exert
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its specific powers, observable by its detergent quality; as has been so long since well explained by Mr. Kirwan. This explanation, however, could not extend to the *potassa carbonata*, or crystallized vegetable alkali, lately introduced, and with equal success. May I be permitted (notwithstanding its use as a test, and *non-deliqescence*) to entertain some doubt of its complete saturation? for that, prepared with the most scrupulous care, still retains its alkaline taste, and acts, with energy, on the vegetable blues. The carbonic may, probably, be too weak an acid, to entirely annul its alkaline property, in any proportion that we can possibly unite them, in the solid state. I am informed by Mr. Kirwan, however, that this can be effected; but the saturation is temporary, and continues only during its most recent state. This we now accomplish, by mechanical means, in the fluid one of our soda, and other mineral waters; where, indeed, the alkali may be considered as in the super-carbonated state. Now, the success attendant also upon their exhibition, completely does away my former hypothesis; and we are left to conclude, that either the opinion, of their want of action, upon gravelly and calculus matter, is unfounded, or that the animal economy may be possessed (among the multiplicity of its wonders) of some unknown chemical agency, whereby it may, in their course through the circulation, disengage their carbonic acid gas.

This would not appear more extraordinary, than the formation of the different and most opposite secretions; such

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as bile, and milk, from one and the same fluid: nor than what we every day observe to pass, in the functions of the vegetable department of the organized kingdom. The *salsola kali*, *salicornia*, and other maritime plants, afforded to Chaptal's analysis, in their early state of growth, muriate of soda: when the plant was more advanced, this salt, with excess of alkali; but in a full state of maturity, the same quite disengaged, and uncombined with muriatic acid. Here, then, we have one of our most refractory salts, and that resists the action of our greatest fires, completely decomposed, by the vegetative powers of an humble plant. In this state of uncertainty, I determined upon a course of some experiments, which might throw some light on this subject; and go to explain how, or upon what principle, alkaline earths, or carbonates, become remedies in those complaints. And here, again, I must bring to our recollection, that whatever retains the uric acid in a state of solution, whilst in the body, must prevent the formation of gravel, or calculus matter of that kind.

And to begin with lime-water, so generally prescribed since the time of White. In this, the quantity of pure earth in solution (being only one grain in 700 of water) is so minute, and it is, withal, so readily decomposed, that we could not, a priori, expect much from its agency.

On conversing, however, with my friend, Dr. Harvey, on this subject, (of whose professional acumen it is unnecessary to make mention here,) he observed, (with that
strength

strength of reasoning peculiarly his own,) “ Whatever may
 “ be the result of your future inquiries, can you, for a
 “ moment, imagine, that physicians of the first eminence,
 “ and of all nations, would still consent to tread in the
 “ path of empiricism, by persevering in the use of this
 “ remedy, if they were not retained in it, by the irresist-
 “ ible evidence of a successful practice and observation?
 “ or that the late Dr. Smyth, a gentleman of great dis-
 “ cernment, and extensive knowledge, would, so generally
 “ and promiscuously, prescribe lime-water, in gout and
 “ gravel, if he were not satisfied of its efficacy, as well
 “ as of the great similarity of these complaints?”

EXPERIMENT I.

To four ounces of healthy urine, was added one ounce of lime-water. A similar quantity of urine was set aside as a standard; both in close vessels: temperature varying from 60 to 75 degrees, being in August, 1799. In the first, no sign of the slightest separation, or crystallization of uric acid, after three, five, or seven days. Some observable in the standard, after the third day, which increased in quantity to the fifth.

EXPERIMENT II.

To the same quantity of urine, was added half an ounce only of lime-water, with the same appearances as before.

No sign of uric acid after several days. The standard deposited a few crystals, on the third morning.

EXPERIMENT III.

To the same quantity of urine, were added two drachms only of lime-water; which, though insufficient to neutralize the disengaged phosphoric acid, yet seemed as effectually to prevent the separation of uric acid, as the greater quantities employed in the former experiments.

EXPERIMENT IV.

To three ounces of the urine of a child, six years old, still warm, and subject to deposit gravel upon cooling, were added two drachms of lime-water; which effectually prevented all separation of this matter; whilst the standard precipitated, copiously, this saline substance, after three hours.

EXPERIMENT V.

To three ounces of the same kind of urine as in the preceding, was added one drachm of lime-water: the result the same as in the former. Some precipitation in the standard, as in the preceding experiment.

When we consider the small proportion of lime kept in solution in water, and that the lime-water, used in my experiments,

experiments, was far from being recent, we must be astonished at the minute quantity, that proves sufficient to keep the uric acid in solution: but this wonder ceases, when we recollect, that the proportion of it, in the above quantities of urine, is extremely small, and that it is scarcely acid; as we may learn from the controversy that took place, between two such able chemists as Pearson and Fourcroy, on this subject. Finding, then, our common lime-water exerting such powers, in preventing the separation or crystallization of this substance, it occurred to me, that much more might be expected from barytic lime-water, as containing a larger proportion of saline matter in solution; and that, though, from its poisonous effects in the carbonated state, the internal exhibition would be hazardous, yet it might prove an useful remedy, when injected into the bladder. But how uncertain are our apparently best founded theories, when not deduced from experiment!

EXPERIMENT VI.

To three ounces of urine, was added one drachm of barytic lime-water, which immediately seemed to decompose the whole, render it turbid, and give it the appearance of the *urina jumentosa*; for reasons easily and satisfactorily explained by Fourcroy, to whom I must refer. After two days, I was surprised to find some small crystalline matter adhering to the sides of the glass; and, upon examining

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the copious precipitate from this re-agent, I found it blended with as much crystallized uric acid, as appeared in the standard. A repetition of this experiment, both since, and at that time, afforded the same results. Now, the strength of barytic lime-water is, to that of the common, nearly as 13 to 1; the former keeping in solution, at the temperature 60 degrees, 13 grains to the ounce. Has the barytic, with all its superior energy, less affinity for the uric acid, than the calcareous earth? Or, does a superior affinity, to some other ingredients of the urinary compound, supersede its union to this?

I regret, that the small quantity of stronthian lime-water, which I possessed, did not permit me to extend my inquiries with it.

Finding, then, that our alkaline earth of lime, in the weakest possible state of solution, and in the smallest proportion, effectually prevents the crystallization, or keeps in solution, the lithic acid of urine: if we only suppose, that it reaches the kidneys and bladder, in the smallest quantity, it must produce similar effects there, obviate the further formation of gravelly matter, or further accumulation of pre-existing calculous concretions of this kind.

Let us now proceed to enquire into the effects of the pure and carbonated alkalies themselves. The action of the former being well known and acknowledged, I shall content myself with one experiment, and pass on to the latter.

EXPERIMENT

EXPERIMENT VII.

To four ounces of the urine of a child, often depositing gravel, on cooling, were added ten drops of the *aqua kali puri* of the shops. After seven days, no sign of separation: some observable in the standard, after some hours.

EXPERIMENT VIII.

To four ounces of urine, were added three grains only of crystallized carbonate of potash, the purity of which was ascertained by Dr. Perceval and myself; and containing, according to Mr. Kirwan, 1,23 grains of alkali. After several days, no appearance of crystallized matter: some in the standard, after forty-eight hours.

EXPERIMENT IX.

To the same quantity of urine, were added two grains only of the same: with the like result.

From the pure and carbonated, let us now proceed to the super-carbonated and sub-carbonated states.

EXPERIMENT X.

To three ounces of urine, in a well closed phial, was added half an ounce of the *aqua mephitica alkalina*, prepared

pared according to Dr. Faulkner's proportions. No crystallized appearance, after seven days. This result we might well expect, from the relative large proportion of its alkaline salt; having already seen equally good effects, from half an ounce only, of Kinsley's soda water, containing a mere fraction of alkaline matter.

EXPERIMENT XI.

To three ounces of urine, were added two grains of the common salt of tartar of the shops; containing, according to Mr. Kirwan, 0,82 of alkali. The same results, as in the former experiments, even after six days.

EXPERIMENT XII.

Having no pure mineral alkali, three grains of the common crystallized soda of the shops, containing, according to Mr. Kirwan, 0,64 of alkali, were added to four ounces of urine. The result as in the former: which was equally produced, by two grains only; and, perhaps, would have been equally so, by one.

Wishing to be more fully convinced, that the very large proportion of disengaged carbonic acid gas, in our soda waters, did not counteract the usual alkaline effects:

EXPERIMENT

EXPERIMENT XIII.

To four ounces of the urine of the same child, which was generally surcharged with gravelly matter, I added half an ounce of Kinsley's soda water, in its full state of effervescence; the phial well corked, and removed into a cold cellar, temperature 42 degrees. After four days, nay, a week, nothing but the usual calcareous sediment, without an atom of crystallized, or otherwise precipitated uric acid.

From the above experiments, then, we learn, that pure lime, in the state of lime-water, the pure alkalies, the sub-carbonated, carbonated, and super-carbonated, all prevent the separation of the uric acid, by uniting probably with, and retaining it in solution. That they should still exert their power, in the super-carbonated soda water, is rather singular: and we must suppose, that, in the temperature of the human body, this superabundant gas (which, for the greater part, is only retained by compression) would be disengaged, and leave the alkali to exert its usual properties; and so, I would presume, it happens.

A half-pint of soda water was poured into a large glass, and exposed to the influence of the atmosphere, in a temperature of from 60 to 75 degrees. After two days, it continued to turn litmus red, and only ceased to do so, at the end of three. But, in Experiment X. we find it in its full gaseous state, still possessing its alkaline influence

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on the uric acid; which I would be disposed to attribute to its very weak union to the carbonic acid, in the fully carbonated and super-carbonated states; as well as to the very weak degree of acidity of the uric acid itself, rendering the most minute portion of all alkaline matter sufficient to its saturation. However this may be, it is obvious, that the extraordinary quantity of gas, with which these waters are surcharged, is undoubtedly superfluous, and may, probably, prove dangerous. In gouty habits (so subject to these complaints) there is always danger of their inducing spasmodic affections of the stomach. This has frequently occurred: and if, to prevent it, we are obliged to add spirituous tinctures, and brandy, why not as well omit this super-saturation at once, and content ourselves with that pleasing degree of it, which exceeds but little that of saturation?

Nor have the predisposed to apoplexy less to apprehend. And, in these cases, we find our own physicians, as well as those of the sister kingdom, preferring carbonated potash, or desiccated soda. But, recollecting that I am acting, on this occasion, the part only of the experimenter, I shall now proceed to consider, what the action of these saline substances may be, on the uric acid, in its concrete or calculous state, as well as on a few others of these concretions; which, though of a different nature, are of frequent occurrence, and easy solubility. The nature of those employed in the following experiments, was always previously ascertained and specified; they were also carefully weighed

weighed and dried, both before and after immersion in their several menstrea.

Dr. Percival, of Manchester, as well as others, having experienced the solvent power of the plain mephitic, or carbonated water, on urinary calculi, it was thought proper to repeat his experiments.

EXPERIMENT I.

A fragment of a calculus, weighing twenty-three grains, and of the uric acid kind, was suspended by a thread, for forty-eight hours, in Nooth's apparatus, already nearly filled with highly impregnated aerated water, and still exposed to a stream of carbonic acid gas: temperature 58 degrees. When taken out, and dried, weighed, as before, twenty-three grains.

EXPERIMENT II.

A fragment of a calculus, of the same kind, weighing forty grains and three quarters, was suspended, as before, in Nooth's apparatus, for forty-eight hours: temperature varying from forty to fifty-five degrees. On being taken out, and dried, was found to have sustained no loss.

EXPERIMENT III.

An entire calculus, of a rough and sandy appearance, chiefly of the uric acid kind, but with some extremely

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minute intermixed particles of the ammoniacal magnesian phosphat, weighing fifty-two grains, was suspended, as before, for forty-eight hours, in Nooth's apparatus. After being taken out and dried, was found to weigh fifty-one grains and a quarter; so that there was here a loss of three quarters of a grain: undoubtedly, of the ammoniacal magnesian phosphat.

EXPERIMENT IV.

Wishing to see, whether even increased temperature would add to the solvent power of carbonic acid, a fragment of calculus, of the uric acid kind, weighing twenty-two grains, was immersed, as before, for forty-eight hours, in three ounces and a half of highly impregnated carbonated water, in a well closed phial, and laid on a sand heat, which did not exceed the temperature of 100 degrees. After being taken out, and dried, the weight was found, as before, twenty-two grains.

From these experiments, then, we may conclude, that calculi, of the uric acid kind, are insoluble in carbonated water; and that Dr. Percival, whose character as a philosopher, as well as a physician, deservedly stands so high, must have operated upon concretions of a different kind; more especially as, in his experiments, there was a loss of several grains, in only a few ounces of mephitic water; whilst none appeared in ours, though in several pounds of
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that fluid. He must have then operated upon some of a different and highly soluble kind.

EXPERIMENT V.

One half of a calculus, of the ammoniacal magnesian kind, weighing 100,5 grains, was suspended, as usual, for forty-eight hours, in Nooth's apparatus: temperature 50 degrees. Upon being taken out, and dried, was found to weigh 92,63 grains; so that its loss amounted to 7,87, or rather more than seven grains three quarters. And here we have an explanation of the result of Doctor Percival's experiments; by supposing that the calculi he employed must have been of this species: the extreme solubility of which, in so weak and innocent a menstruum, should excite the earnest hope of our young gentlemen, in the surgical department, of effecting its solution, by injections of carbonated water into the bladder. To such safe trials they must be encouraged, by the pleasing consideration, that this kind forms a large proportion of the human urinary calculi. On these occasions, the water should not be too highly impregnated; lest the sudden expansion of the gas, under the human temperature, might excite the bladder to reject it too quickly. This inconvenience, however, must be in part obviated, by the necessity of previously warming the injection, to about the temperature of 80 degrees: a precaution never to be omitted. But to return to the alkaline earths and salts.

EXPERIMENT VI.

One half of a uric acid calculus, was suspended for forty-eight hours, in four ounces of lime-water: temperature as before. After being dried, was found to have lost seven grains three quarters; and the surface to be covered with a granular efflorescence, which, in drying, detaches itself. The calculus was so much softened, as to leave little doubt of its entire destruction, by a few more immersions. It was again suspended, for a month, in the same quantity of lime-water, in the temperature of the atmosphere only, without any renewal of the menstruum; when it was found to have lost twenty-four grains. Now, if the lime-water had been frequently renewed, and its energy assisted, by the standard heat of the human body, no doubt but it would have been entirely broken down, in a much shorter time. We find, then, lime-water, not only preventing the separation of uric acid from urine, but acting powerfully upon it, in its most compact form. How well founded, then, were the experiments of Whyte, as well as the opinion of Doctor Smyth; and how little deserving the latter the obloquy of his contemporaries, for his predilection to it. Now, this result, corresponding also with Scheele's, points it out to us, as one of our most safe and active agents, when injected into the bladder, with the necessary precautions. And we must feel surprised, that
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no attempts of that kind have been made, since the time of Whyte.

EXPERIMENT VII.

A fragment of calculus, of the uric acid kind, weighing seventy-nine grains and a quarter, was suspended, for forty-eight hours, in a mixture, consisting of four ounces of distilled water, and twenty-five drops of the weak *aqua kali puri* of the shops, which merely gave it an alkaline taste. It was then placed on a sand heat: temperature varying, occasionally, from 60 to near 100 degrees. After being taken out, and dried, it weighed seventy-four grains and three quarters; so that the loss amounted to four grains and a half. This weak lixivium, it appears, then, operated upon it, acquired a yellow colour, a sweet taste, and precipitated, with a few drops of muriatic acid, a white sediment, easily recognizable, by the small, silky, needle-shaped, crystalline appearance, peculiar to the uric acid.

EXPERIMENT VIII.

This same fragment of calculus, after being well washed, and dried, was again immersed, for forty-eight hours, in a lixivium, consisting of only twenty drops of *aqua kali puri* to four ounces of distilled water, and under the same circumstances. Upon being taken out, and dried, it was
found

found to have lost four grains and a quarter. The solution was of a yellowish green colour, lost all alkaline taste, and precipitated, as before, with either the acetous or muriatic acid.

EXPERIMENT IX.

A fragment of calculus, of the same kind, weighing eighty-one grains, was suspended, as before, for forty-eight hours, in a mixture, consisting only of fifteen drops of the same alkali, to four ounces of distilled water, which scarcely imparted an alkaline taste. After being taken out and dried, it was found to have lost one grain and three quarters; the specimen consisting chiefly of the external laminae, much more slowly acted upon.

EXPERIMENT X.

This same fragment, washed and dried, was again immersed, for forty-eight hours, in a similar lixivium, and under the like circumstances. The loss now amounted to nearly four grains; and, from this we learn, how considerably the energy of the menstruum is increased, by each succeeding immersion; so much so, indeed, that a few repetitions enable it to disunite the laminae, and cause them to crumble into a pulverulent state, easily voidable with the urine. To the happy result, therefore, of this experiment, let me earnestly solicit the attention of our young practitioners.

EXPERIMENT

EXPERIMENT XI.

As children are such frequent sufferers, Mr. Richards suggested the propriety of ascertaining, whether the alkaline influence might be weakened, by the addition of sugar. One half of a calculus, of the uric acid kind, weighing $185\frac{1}{2}$ grains, extracted by my friend Mr. Richard Dease, and (though under the most unpromising circumstances) with a dexterity and success, not to be exceeded by his late father, was suspended in a lixivium, consisting of eight ounces of distilled water, and twenty drops of weak *aqua kali puri* (partly aerated), and scarce imparting an alkaline taste. To this were added thirty-six grains of sugar, which were found adequate to sweeten it sufficiently. After remaining forty-eight hours, in a temperature, varying from 55 to near 100 degrees, or a medium one of 74° , being dried and weighed, it was found to lose ten grains three quarters. The addition, then, of saccharine matter cannot diminish, but may add to the alkaline energy.

EXPERIMENT XII.

Ten grains of very pure crystallized carbonate of potash were dissolved in four ounces of distilled water. In this filtered lixivium was suspended a fragment of calculus, of the uric acid kind, weighing seventy-two grains and a quarter, for forty-eight hours, on a sand heat, varying from 50
to

to 100 degrees (for the fire was not kept up during the night). Being taken out, dried, and weighed, it was found to have lost seven grains and a quarter. The solution had a yellowish green colour, different from the light yellow tinge of the pure alkaline ones. It also lost its taste, but without becoming sweet. A quantity of flocculent animal matter was separated, and the dissolved uric acid was, for the greater part, again precipitated, upon the mixture cooling, to the temperature of the atmosphere.

EXPERIMENT XIII.

The crystallized carbonate of potash, being generally prescribed, in the proportion of one drachm to four ounces of water: in a similar mixture was suspended an entire calculus, of a very compact, rough, and gritty appearance, weighing forty grains and a quarter. After remaining forty-eight hours in the above temperature, it was taken out, dried, and weighed, and found to have lost three grains three quarters. The solution here more highly coloured than in the former: some spontaneous precipitation; and an immediate one, on the addition of a few drops of weak marine acid. We then find the vegetable alkali in the fullest state of saturation, with carbonic acid, that we can procure it in the solid form, acting powerfully on these concretions, when assisted by degrees of temperature, even much inferior to that of the human body.

Now,

Now, as to the mineral alkali, nature presents us with similar, nay, more extraordinary results, in the mild, mineral, alkaline impregnation of the waters of Carlsbad in Bohemia. Here are several springs, varying in temperature from 114 degrees, to that of the Brudel, at 165°. According to Elliot, they contain, in the gallon, of aerated lime 36 grains; muriate of soda 48; aerated soda 102; vitriolated soda 6 drachms; some minute proportion of iron, and a considerable carbonic acid impregnation. But Klaproth rates the proportion of mineral alkali still higher.

Of the lithontriptic effects of these waters, Springfield gives us a very surprising account indeed: founded, however, upon numerous experiments, instituted upon the spot, by the immersion of many calculi, in the sources themselves; where they were either entirely dissolved, or acted upon, with an energy that must appear incredible, if we did not consider the nature of the menstruum, its high temperature, and constant renewal, by the flowing of the stream. Nay, the urine of patients, who used these waters for a few days, was found to possess powerful lithontriptic effects; as appeared by the immersion of many calculi in it. For an account of these highly interesting experiments, too numerous for insertion here, I must beg leave to refer to his *Treatise De Prærogativa Thermarum Carolinarum, in dissolvendo Calculo Vesicæ, præ Aqua Calcis vivæ*.

From these experiments, as well as the highly beneficial effects of these waters, taken internally, by the numerous calculous and gravelly patients, who frequent Carlsbad, he

establishes their superiority over the different alkaline and other remedies, hitherto in use; not excepting Whyte's oyster-shell lime-water. Now, the lime in these being carbonated, and only kept in solution by their highly aerated state, we can be at no loss, in those days, to attribute their superior agency to the alkaline impregnation, assisted by so high a temperature. Klaproth affirms, that a person, who drinks these waters, in the usual quantity, for twenty-six days, takes of mild mineral alkali 3913 grains, or 8 ounces, 1 drachm, and 13 grains; which amounts to two and a half drachms per day, besides the other saline ingredients.

Doctors Rutty and Smyth, who gave us a valuable extract from this publication, in the *Memoirs of the Medical and Philosophical Society of this city*, (now in the library of the Royal Irish Academy, but which, we have sincerely to regret, were never published, and are now discontinued,) conclude their account by the following query. "May not some alkaline lixivium be contrived by art, that would possess similar effects with these waters?" And has not this partly taken place, in the instance of our soda waters? But may we not make a nearer approximation, by a solution of the above specified proportion of mineral alkali, in the relative quantity of water, with the addition or omission of the carbonic acid, and the other saline ingredients, as may be thought proper; afterwards heating, however, each separate dose to 160 degrees?

We find, then, the alkaline carbonates, in the great laboratory of nature, as well as in our experiments, exerting considerable

considerable solvent powers upon these animal concretions, contrary to what has been hitherto supposed.

EXPERIMENT XIV.

Into a filtered solution of ten grains of salt of tartar, in four ounces of distilled water, were introduced two fragments of calculi, weighing seventy-four grains and a quarter. The mixture was set aside for forty-eight hours in a cool room; temperature varying, from 47 degrees at night, to 55 degrees in the day. After twelve hours it began to be coloured, and continued to be more so, until the temperature fell to 51 degrees, when a precipitation took place, and continued during the night; so that it appeared to deposit, at the temperature of 47 degrees, what was taken up at degrees somewhat exceeding 51°. These fragments, on being taken out, dried, and weighed, were found to have lost three grains and three quarters; the laminæ disposed to crack, and the strata to separate and crumble. This weak lixivium, then, exerted much energy, even in a very low temperature.

EXPERIMENT XV.

A fragment of calculus, weighing seventeen grains three quarters, was immersed in a lixivium of similar strength; but now exposed to a temperature, varying, from 51 degrees at night, to about 95 degrees in the day. After

forty-eight hours, it was found to lose five grains and a half: a prodigious quantity, when we consider the small surface presented by this fragment, weighing only seventeen grains three quarters. The solution, upon cooling, became turbid, as before, and precipitated a large proportion of the dissolved uric acid.

EXPERIMENT XVI.

A fragment of calculus, weighing forty grains three quarters, was immersed in four ounces of soda water for forty-eight hours, and exposed to a temperature, varying from 55 to about 100 degrees. Its loss amounted to one grain. A repetition of this experiment afforded nearly the same result; and demonstrates, that though the soda, in this super-carbonated state, still exerts some energy, on concretions of the uric acid kind, yet it is but feeble; and that these waters appear more capable of preventing their formation, than effecting their solution, when they once acquire the aggregate state. The same fragment, in a similar quantity of soda water, in the temperature of from 50 to 55 degrees only, sustained no loss, after forty-eight hours. And here we have another proof of the necessity of seriously attending to the degree of temperature, in all researches of this kind.

But it may be observed, as to the internal use of alkaline substances in particular, that their effects must be considerably weakened, upon their immediate admixture
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with the urine: as the small quantity, that can be conveyed there, must, in the first place, neutralize the uncombined phosphoric acid, in all urine; the benzoic, in children's; and decompose the ammoniacal and magnesian phosphats, in that of every period of life. It must be acknowledged, its efficacy is partly counteracted by these circumstances, which should never be overlooked, and always taken into account, in practical application. Referring to Fourcroy's instructive essay on this subject, *Memoires of the National Institute, and Connoissances Chimiques*, let us here once more appeal to the test of experiment.

EXPERIMENT XVII.

A fragment of calculus, weighing eighteen grains one quarter, of the uric acid kind, was suspended, for forty-eight hours, in an alkaline lixivium, consisting of four ounces and a half of recent urine, and twenty drops of a very weak, and partly aerated, caustic lixivium: medium temperature about 74 degrees. On being taken out, and dried, it was found to have lost one grain three quarters: a considerable quantity, from so small a specimen. To the filtered solution, were added a few drops of dilute marine acid; which, after a few minutes, precipitated a reddish crystalline matter, in a triple proportion of what generally occurs in the natural state of urine.

From the above experiments, therefore, it appears no longer doubtful: First; that pure lime, even in the small
proportion

proportion contained in lime-water; and the pure alkalies, in an extreme state of dilution, in temperatures even somewhat inferior to those of the human system; exert an active solvent power on calculi of the uric acid kind. Secondly; that the alkaline carbonates, under similar circumstances, are possessed of similar powers, though in an inferior degree. And Thirdly; that, by our having ascertained this point, we have removed a long established error, substituted a discovery highly interesting to animal chemistry, and likely to be productive of a more enlightened and successful practice, in the treatment of these diseases.

In these expectations we will appear to be the better founded, when it is considered, that, for want of entire specimens, (preserved here like the oriental bezoars of old,) we were obliged to operate upon fragments, presenting small surfaces only to our solvents: that these last were never renewed during the course of the experiments, which would not have occurred in their application in the form of injections; as they should, in that case, be so often repeated, and act, of course, with renewed energy: that, either taken internally, or used in form of injection, the smallest proportion of alkaline matter, in a great state of dilution, assisted by the human temperature, answers our purpose: and that the temperature, in our experiments, was never permanent, and might be rated at the medium one of 74 degrees.

Having now fulfilled the second object of this essay, I would no longer presume to trespass on the indulgence
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of the Academy, if I were not actuated by the sanguine hope, of turning the attention of my surgical friends, to the humane consideration of obviating, as much as possible, the most dangerous of operations, by the prudent application of a few safe solvents, injected into the bladder. How far they may succeed, with calculi of the uric acid kind, may be already conjectured, from the preceding experiments: but with those of the next most frequent occurrence, there is much less difficulty to encounter, and every reason to hope for a speedy and safe result. The ammoniaco-magnesian phosphat is partly soluble in water; highly so, in the carbonic acid, (as we have already seen;) and, consequently, more so, in the weakest possible acid impregnations that can be devised: nothing more being necessary, than the addition of as many drops only of weak muriatic acid, as will scarce impart an acid taste. But as precept should, in every instance, be as much as possible assisted by experiment, I shall, for the encouragement of the young practitioner, exhibit a few, on this very soluble species, the more willingly, as he has no assistance to expect from his professional books: these subjects being only treated of in Philosophical Transactions, Memoires of the National Institute, and a few other foreign chemical publications, if we except Whyte's Treatise on Lime-water, to which we would willingly refer him.

EXPERIMENT

EXPERIMENT XVIII.

An entire calculus, of a reddish, gritty appearance, externally, proved to consist of ammoniaco-magnesian phosphat, weighing forty-six grains one quarter, was suspended, for forty-eight hours, in a mixture, consisting of four ounces of distilled water, and ten drops of weak marine acid. After being taken out, and dried, it was found to have lost six grains three quarters. The mixture was whitish, lost its acid taste, and precipitated, on the addition of a few drops of fixed alkali, the ammoniaco-magnesian phosphat, under that beautiful crystalline form, so accurately described by Doctor Wollaston.

We will readily conceive, how much more the loss would have amounted to, in this case, in the short space of forty-eight hours, if the menstruum had been frequently repeated, under the regular influence of human temperature.

EXPERIMENT XIX.

A fragment of the same species with the above, weighing twelve grains, was immersed, for forty-eight hours, in three ounces of distilled water, without addition: temperature from 60 to near 100 degrees. After being taken out, and dried, it was found to have lost one grain three quarters, became so friable as to crumble, and the solution to precipitate with a few drops of pure ammonia. This species

cies of calculus, therefore, is soluble in water, at temperatures even inferior to that of the human. It is unnecessary I should enter into a further detail of experiments, made upon calculi of the mixed kind, having the uric acid, phosphat of ammonia, and, sometimes, though rarely, phosphat of lime, intermixed in their strata. Suffice it to say, that the very dilute marine acid speedily takes up the earthy phosphats, leaves the laminae of the uric acid bare and distinct, ready to crumble, and of easy solution, in the weakest alkaline lixivium; and still more so in lime-water. A most important consideration, in a practical point of view.

It would be trespassing too much, on the already tried indulgence of the Academy, to go further into the detail of the circumstances necessary to be attended to, and acquainted with, to insure success in the application of these principles. These are already tolerably well detailed, in the *Connoissances Chimiques*. To the gentlemen, professors in the school of surgery, it more particularly belongs: and, from the zeal and talents now in full activity there, what may not be expected? Created only the other day, by a Cleghorn, (a name as deservedly as universally revered;) fostered, afterwards, by the anxious care and talents of Mr. Dease; we find it already arrived at a state of perfect maturity, and holding out, to the student, advantages, no where to be rivalled, if indeed equalled: and, that nothing may be wanting, to a complete medical, as well as surgical education, establishing a chair of botany,

supported by the acknowledged abilities of Doctor Wade, both as a botanist and teacher. From the above experiments and observations, we may presume to draw the following conclusions.

That acids, and acescent drinks of all kinds, give rise to gravelly and calculous affections, by causing a separation, and precipitation, of the native uric acid of urine within the body. That all acids, vegetable or mineral, nay, the native phosphoric acid of urine, in excess, are equally productive of this effect; the tartaric, perhaps, somewhat more so. That, on the other hand, we find lime, both the fixed alkalies, pure as well as aerated, (even in the smallest proportions,) serviceable in these disorders, by uniting with, and keeping in solution, this acid substance. That they, also, in the smallest proportions, and diluted state, exert strong solvent powers on this acid, in its aggregate form of calculus; provided their action be favoured by degrees of temperature approaching to the human. That, under the same circumstance, contrary to what was generally supposed, the carbonated, sub-carbonated, nay, the super-carbonated, exert similar influence, though in an inferior degree. That lime, even in the small proportion it presents itself to us in lime-water, is a most active and safe solvent of calculi, of the uric acid kind, and its various combinations; as has been long since ascertained by Whyte. That, weight for weight, it exceeds even the caustic alkali, in any state of dilution that the latter can be applied to the living body. That, finding
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four ounces of lime-water, containing only two grains three quarters, take up, or detach, seven grains three quarters, from a very compact calculus, we may be led to suppose this may arise from its action on the agglutinating medium; its affinity to, and energy on animal matter, being so well known: and, if so, may we not expect something, from its power on the mulberry calculus, our most formidable enemy? For, though it cannot touch the oxalt of lime, it may the cementing medium, with which it peculiarly abounds.

For the application of these established facts to useful purposes, I must refer to my surgical friends, being all now possessed of the necessary degree of chemical acquirement; and I am happy to find this career already entered on, by my friend Mr. Crampton, who has favoured us with an analysis of a pulmonary calculus, in the Philosophical Transactions; and from whose professional, as well as scientific talents, we have every thing to expect, in fulfilling (even on this occasion) his duties as a teacher.

Having now endeavoured to accomplish the chief object of this Essay, which was, to establish experimentally a more clear and comprehensive view, of the nature of these maladies, and the remedies employed to combat them, than we hitherto possessed; I should not have trespassed further on the time of the Academy, were it not properly suggested, by my friend, Dr. Clarke, that it would be of importance to ascertain, how far the facts and notions, brought forward in it, may stand confirmed or contradict-

ed, by the result of our practical application of them, in Simpson's Hospital: an establishment affording the best and most extensive field of observation, of this kind, of any in Europe, that of Luneville, perhaps, excepted.

The benefit of this charity extends equally to the blind and gouty. In the year 1795, I found it to contain thirty-two of the latter: and, since that period, thirty-four have been admitted; in all sixty-six gouty patients. Of these, the greater number have either complained of gravel, or passed it, without any previous or concomitant inconvenience: a circumstance which I had, every day, occasion to observe, whilst attending to the state of gouty urine. Among the blind and gouty, however, we may count about twenty-two, as specifically more afflicted: having occasionally complained of marked and distinct symptoms of this disorder. Of these, we find sixteen among the gouty, and six, only, among the blind. Now, as the severity of gout is uniformly diminished, nay, in many instances, the disease entirely removed, by a residence of a few years only, in the house; we must expect to find the same take place, with respect to gravel, to which it is so strongly and nearly connected. And this singular alleviation of both diseases we can only attribute to the influence of temperance, and a manner of living, very opposite to that of their former habits. The diet, in our house, consists of bread and milk, for breakfast and supper; beef or mutton, with table beer, for dinner; all of the best quality, and administered with the greatest propriety and regularity;

rity; whilst the introduction of ardent spirits is prohibited, and sobriety enforced, by the strict discipline of the house. On the other hand, we find that, previous to their admission, they were either addicted to intemperance, or in the habit, at least, of muddling in public houses; where, after a libation with porter, they indulged in the free use of acidulated punch, (the constant nocturnal practice of our middling tradesmen and shop-keepers, who furnish the greatest proportion of our patients). The keeper of a porter-house, of considerable resort, informs me, that, to please the generality of his customers, he finds it necessary to add the juice of an entire lemon to about two quarts of punch; and that, from this circumstance, he would have experienced a considerable diminution of his profits, if he did not occasionally substitute cream of tartar, or the dilute sulphuric acid: an innocent and safe practice, in his opinion. Now, so satisfied are our patients, of the pernicious effects of acids of all kinds, that we find many of them refuse to make use of our table beer, during the summer months; through the apprehension of its acescent quality, (as was before observed;) and which continued to be the practice of Hewson, Khensk, Clapham, and others, for years back: nor do our present two greatest sufferers, Sing and Cox, venture on it, at any season, but with the greatest caution.

To a removal, then, from the former occasional causes, we may attribute no small share of the alleviation of
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those diseases which takes place with us: a practical observation, that cannot be too generally known. But to return to my subject. On the slightest appearance of gravelly symptoms, unconnected with fever, or inflammatory tendency of the urinary system, our patients have recourse to an alkaline medicine, the gravelly pills, (as they term them;) which consist of desiccated soda, in the most convenient form for hospital practice, as well as most suited to gouty stomachs. Of this, (as first advised by Beddoes,) one drachm, with the addition of a few grains of capsicum, or drops of essential oil, and the necessary quantity of hard soap, or extract, is made into twenty pills. Of these, from three to six, or more, are taken in the twenty-four hours; and are found sufficient, not only to alleviate or remove these complaints, but even to render the interference of the physician but seldom necessary. We have had also occasion to remark, that several of our patients, induced by their marked beneficial effects, carried these pills about them, so as to have occasional recourse to them, without much attention to either dose or number.

To this practice, then, we would be disposed to attribute, the very pleasing and interesting consideration, that, among so many gravelly patients, there has not occurred, in the course of ten years, a single operation of lithotomy; nor has the catheter, even in the hands of our expert and able surgeon, Mr. Macklin, been able to discover the smallest occasion for it. We could therefore, have no opportunity

opportunity of ascertaining the efficacy of injections into the bladder, as recommended by Whyte, Fourcroy, and myself.

I shall conclude by observing, that it would be interesting, to have it in our power to extend these researches, to the urine of those, who live habitually on different aliment and drinks, particularly of the acescent kind; as well as to that of those, who drink waters, with mineral alkaline impregnations. But this desirable object can be only obtained, by the concurrent exertions and attention of gentlemen of the Faculty, in different countries and situations. In private practice it is not to be expected: for here, wherever experiment is surmised to be the object, mistrust and suspicion take place of professional confidence. The use of the nitric acid, in our venereal hospital, I hoped, would afford some useful facts, as to its effects, upon the saline contents of urine; the uric acid in particular. But I had not, as yet, sufficient leisure for that enquiry; nor could I, hitherto, obtain the urine of those using it, with all the circumstances, necessary to enable me, at this moment, to draw any direct conclusions, from my examination of it. In many instances, a morbid state of the urinary system (the urethra in particular) took place. In others, the combined effects of mercury interfered: and in all, no certainty of its not being blended with the urine of others, not using this acid. I could not, however, help observing, that the few specimens, sent to me, agreed in one particular, viz. their exceeding very little,

little, if at all, the usual healthy standard of acidity. This circumstance must excite our attention the more forcibly, when we consider, that two drachms of nitrous acid, nay, sometimes three, diluted in the proportion of one pint of water to each drachm of acid, were taken daily; whilst, on the other hand, a few drops of the acid elixir of vitriol, or *tincturæ martis in sp. salis*, nay, the weak vegetable acids, and cream of tartar, persevered in for a few days, impart an additional degree of acidity to the urine. Would not this observation (if founded), conjoined with the easy decomposable nature of the acid itself, and its action on animal matter, induce us to lean to the opinion of those, who have already asserted, that this acid is partly decomposed in the system, imparts its oxygene to it; and that, perhaps, to a degree, capable of annulling or destroying its properties as an acid?

And it may be here further remarked, in confirmation of such notion, that those gentlemen, most conversant with it here, as well as most capable of judging, entertain strong doubts of its supposed diuretic effects; allowance being made, for the necessary quantity of its watery vehicle. If it be, then, truly deoxygenated in the system, why be deterred by its failure, as a radical cure of syphilis, from extending our trials with it here, to other chronic diseases, as they have already done, in India?

I have, in the former pages, expressed a wish, that physicians should attend to the influence of acescent aliments and drink, upon the state of urine of those who habitually used them; as well as of alkaline matters, upon that of such as occasionally must have recourse to these substances. For it is obvious, that observations and experiments, of this kind, must be more decisive, and less subject to lead us into error, than the most accurate that could be devised, upon this, or any other of the human fluids, when once excreted out of the system, and no longer acted on, by the powerful agency of the principle of life itself.

This desirable object, I now find, has been, in part, already fulfilled, by the industry and ability of Mr. Alexander Philip Wilson, who, in a publication on the *Remote Causes of Gravel*, published in Edinburgh, so far back as 1792, and dedicated to Doctors Black and Monro, (but which I have to regret, only latterly fell into my hands,) details a number of interesting experiments, made upon himself and others. The result of a few of these, as not only highly important in themselves, but as naturally connected with mine, instituted out of the body, I must request the indulgence of the Academy briefly to relate.

From his first experiment, upon himself, it appears, that the natural deposit of uric acid, from a given quantity of his urine, whilst living, as usual, on a diet of vegetable and animal matter, amounted to one grain and a half;

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but was increased to three grains and a half, upon eating four lemons in the twenty-four hours.

From the second experiment, upon a healthy boy, æt. 15, living upon an acescent diet, of bread, milk, sugar, sour cream, and honey; that the natural deposit, varying from three quarters of a grain, to one grain and a half, totally disappeared, upon the use of a diet chiefly animal; and was only observable, where indigestion of the acid kind supervened upon so unusual and unnatural a regimen.

The third experiment, we find merely corroborative of the former. But, from the fourth, as well as some others, we learn, that even acid ingesta, or lemon juice, where the action of the skin is vigorous, and assisted by exercise, contribute very little to the increase of the uric acid deposits of urine; and from this and other experiments, instituted upon himself, as well as others, while under the influence of sudorific medicines, he concludes, that the uric acid passes off, (in part at least,) by the skin, under the circumstances already mentioned.

From the fifth, on the same boy, that, whilst living for twenty-four hours, chiefly on beef and salt fish, (though with an allowance of small beer and potatoes,) no deposit of uric acid took place; but, upon turning over, for the same interval of time, to bread, milk, apple-dumplings and sugar, it amounted to one grain.

From the sixth experiment, upon a healthy boy, æt. 20, that, living entirely on vegetable matters and milk, and
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eating three lemons in the day, a deposit of two grains of uric acid took place; which entirely disappeared, upon his omitting the lemons, and living chiefly on animal food.

From the seventh experiment, on the same person, that a diet of animal food, for forty-eight hours, was productive of no deposit; but amounted to half a grain, upon living, during the same interval, upon a diet, chiefly vegetable, with the addition of three lemons.

I shall finally conclude with the result of his tenth experiment, upon a young man, æt. 19, in good health. Here the natural deposit, of one grain, was increased to two, by eating one pound and a half of apples only. In confirmation of the result of this experiment, I beg leave to add, that, in the course of my attendance upon children, I had often occasion to observe, that this acid excretion was considerably increased, by the use of grapes, persevered in for a day or two only.

We may now, therefore, presume to assert, from the medical history of this disease, as delivered in the first part of this essay, as well as from experiments, instituted in, and out of, the body; that acids, and acescent ingesta, may be considered, as remote causes of gravelly complaints.

ERRATA.



- Page 229, line 4, from the top, for CALCULUS read CALCULOUS.
 — 232, — 3, from the bottom, for Insipidity read Insipid.
 — 235, — 6, —————, for ammonico read ammoniaco.
 — 236, — 9, —————, for ammonico read ammoniaco.
 — 242, — 3, from the top, for calculus read calculous.
 — 247, — 7, from the bottom, for Beverovie read Beverovic.
 — 253, — 14, —————, for carabonates read carbonates.
 — 255, — 3, —————, for excreated read excreted.
 — 280, — 10, —————, for lithic read uric.
 — 308, — 3, from the top, for nitrous read nitric.
 — 308, — 4, from the bottom, for deoxyginated read deoxygenated.